1854MNRAS..14..246J

Places of Comet II. of 1854, *as observed at Madras with the Lerebours Equatoreal.* By Captain W. S. Jacob.

1854.	Madras Sid. Time.	R.A.	N.P.D.	Star compared.	Power used.
	h m s	h m s	° ′ ″		
April 5	8 14 0 (*a*)	2 42 39·0	74 28 0	Independent	113
6	8 7 20	54 22·8	75 27 0		80
7	7 55 0 (*b*)	3 5 13·8	76 27 6		113
8	8 4 0	15 33·20	77 27 41	B.A.C. 1087	113
10	7 55 31 (*c*)	34 6·35	79 28 7	1174	113
11	8 5 20	42 34·67	80 26 39·6	W. iii. 841	83
12	8 9 37	50 29·27	81 23 6·0	903 & 990	—
13	8 8 53	57 50·70	82 17 31·5	1087 & 1147	—
14	8 22 30	4 4 49·65	83 10 27·0	W. iv. 41	—
15	8 20 50 (*d*)	11 19·02	84 0 40·0	170	—
18	8 45 22	28 44·78	86 19 50·0	608	—
19	8 44 55	4 33 53·74	87 1 44·0	695	—

Assumed Apparent Places of Comparison Stars.

Star.	R.A.	N.P.D.
	h m s	° ′ ″
B.A.C. 1087	3 22 47·80	77 33 58·0
1174	40 14·80	79 18 37·9
W. iii. 841	43 21·94	80 23 24·5
903	46 49·70	81 15 19·0
990	50 51·25	81 23 44·0
1087	56 0·90	82 12 38·0
1147	3 59 0·60	82 12 23·0
W. iv. 41	4 3 12·80	83 5 47
170	9 14·24	84 1 6
608	28 16·90	86 14 6
695	4 31 53·90	87 4 24

" (*a*) Nucleus bright, in form of a crescent; light = a star 5th mag.; outline of envelope a fine parabola; extremity undefined. From the low altitude and hazy sky on this and the two following days, no star could be found for comparison, and the places are uncertain to about 30″.

" (*b*) Nucleus only, seen through clouds.

" (*c*) Sky clear; bright moonlight; envelope barely visible, nearly 1° long; nucleus more concentrated, reddish, with ill-defined *horns* towards W.

" (*d*) Wind so violent and gusty that the chronometer-beats could not be heard. Observations otherwise good; instrument being perfectly steady."

On the Difference of Longitude between the Observatories of Brussels and Greenwich, as determined by Galvanic Signals.

By G. B. Airy, Esq., Astronomer Royal.

This Memoir is divided into six Sections.

Section I. *History and General Arrangements.*

" The increasing importance of galvanic communication in the

1854MNRAS..14..246J

operations of Observing Astronomy and the completion of the line of submarine telegraph from the South Foreland to Sandgate, induced me, in the winter of 1851, to solicit the sanction of the Lords Commissioners of the Admiralty for the establishment of a galvanic system at the Royal Observatory of Greenwich, and of wires connecting it with the principal telegraph offices in London. Their Lordships were pleased immediately to grant the necessary authority; and the Directors and Officers of the South-Eastern Railway Company and of the Electric Telegraph Company gave every assistance in their power. The wires were, in consequence, laid in the summer of 1852. Although (in consequence of a change in the relations of the Telegraph Companies) these wires were not used as was at first contemplated, for communication with the Continent; yet their establishment was of great importance, as giving me communication with all parts of Britain, and as enabling me to make the proper arrangements for satisfactory use of galvanic communication by any system of wires. The first determinations of difference of longitude from Greenwich (which, as far as the galvanic operations were concerned, were perfectly satisfactory) were those of Cambridge and Edinburgh, in the summer of 1853; both which were made with these wires.

" In the mean time, the Submarine and European Telegraph Company had laid another system of submarine wires from the South Foreland to Ostend, communicating by the Belgian telegraph wires immediately to Brussels. They had also laid a number of subterraneous wires from their Office in Cornhill to the commencement of the submarine lines at the South Foreland. The subterraneous line passed by the side of the Dover Road across Blackheath, at a distance of less than half a mile from the Royal Observatory. Upon my making application to the Chairman and Directors of the Submarine and European Telegraph Company, permission was immediately given me to connect a wire from the Royal Observatory with one of the Company's wires."

The author then proceeds to describe the way in which this connexion was effected, not only for explanation of the course pursued in the observations detailed in the Memoir, but also for the purpose of showing the liberality with which the Company acceded to his views and the trust which they have placed in him.

" The interest with which M. Arago and other members of the French Académie des Sciences had in 1851 urged the determination of their difference of longitude, and the priority of completion of the submarine telegraph to the French coast, had induced me to contemplate the investigation of our difference of longitude with Paris, as the first use to be made of our connexion with the Submarine Company's wires; and in the summer and autumn of 1853 negotiations for that purpose were in progress with the Bureau des Longitudes. These were interrupted by the illness of M. Arago and other causes. I then considered myself at liberty to commence with the longitude of Brussels; and on my communicating with M.

1854MNRAS..14..246J

Quetelet, I found him most anxious to proceed with the enterprise. At the representation of M. Quetelet, and with the sanction of the Belgian Government, a wire was laid from the Telegraph Office in the *Station du Nord* at Brussels to the Observatory, and the good offices of the Telegraph Company were assured for making the wire connexions at the proper hour of the evening.

"A galvanic telegraph-needle was mounted in the Observatory in close proximity to the transit-clock, nearly as in the Greenwich Observatory. The signals to be made were simple deviations of the needle, produced directly by the galvanic current through the long communicating wire; and the observation was to consist merely of careful observation and register of the clock-time of each deviation of the needle.

"It was arranged that the observations should be divided into two series: that in the first series an observer from Brussels (M. Bouvy) should observe both the galvanic signals and the transits for correcting the transit-clock at Greenwich, while an observer from Greenwich (Mr. Dunkin) made the corresponding observations at Brussels; that this series should be continued till at least three evenings' observations, satisfactory in the determination of clock-correction, as well as in the record of the signals, should be obtained; that the observers should then be reversed, and the second series be observed in the same manner. The signals were to occupy one hour in each evening, from 10^h to 11^h Brussels Mean Solar Time ($9^h\ 43^m$ to $10^h\ 43^m$ Greenwich, nearly), each hour being divided into four quarters. The contacts of wires for completing galvanic circuit were to be made at Greenwich and with a Greenwich battery in the first and third quarters, and at Brussels with a Brussels battery in the second and fourth quarters (or *vice versâ*), and between the two sets of observations at each place the poles of the battery were to be reversed.

"It was laid down as indispensable that the persons who observed the signals should not make the contact by which the circuit is completed and the signals are given.

"The following was the routine of an evening's observations of signals in the first series. In the second series everything was similar, except that Brussels commenced instead of Greenwich. By 'warning signals' are meant signals at intervals of 3^s nearly, intended only to be counted; by 'observation signals' are meant signals at intervals of 14^s or 15^s, intended to be accurately observed.

"In the first quarter of hour Greenwich gave four warning signals, as token of readiness. Brussels answered by four warning signals.

"Greenwich gave two warning signals, signifying 'transits,' and then gave warning signals equal in number to the transits of stars observed in the last preceding evening after galvanic signals. If no transits had been observed, the two signals were omitted.

"Greenwich gave observation signals in groups of seven to ten in

1854MNRAS..14..246J

number, each group being preceded by a similar number of warning signals.

"In the second quarter of hour Brussels gave signals like those of Greenwich in the first quarter (omitting the four signals).

"In the third quarter of hour Greenwich gave two warning signals for 'transits,' and then gave warning signals equal in number to the transits of stars observed this evening. If no transits were observed, the two were omitted.

"Greenwich gave observation signals in groups, with warning signals as before, the poles of the battery being reversed.

"On the day on which, in the opinion of the Greenwich observers, the operation might properly terminate, Greenwich gave fifteen warning signals.

"In the fourth quarter of hour Brussels gave signals like those of Greenwich in the third quarter."

The transit-clocks were corrected by two distinct methods, which were originally suggested by Professor Challis, in the operations for determining the longitude of Cambridge.

"In method A transits of fundamental stars were used, their right ascensions being taken for the reductions at both observatories from the Greenwich Fundamental Catalogue. The stars employed were not necessarily the same at the two observatories.

"For method B two lists of stars were proposed, one preceding and one following the signals. The stars' places were not supposed to be at all exact, but before using them for correcting the clock the lists of stars observed at the two observatories were compared, and all were rejected which were observed at only one observatory. In this manner the transits of strictly the same stars were compared, and the correctness of their assumed right ascensions was unimportant."

The determinations of the collimation error, level error, and azimuthal error of the transit-instruments were referred to the judgment of the superintendents of the two observatories.

"I think it probable," says the author, "that a course generally similar to that described above will be found convenient in any future operation of similar character. In the method of giving the signals, however, I have at this time (August 1854) made a great improvement. By means of an auxiliary clock, the circuit is automatically completed, and the signal is given at every 15^s, as shown by that clock. The approximate knowledge of the time of signal enables the observer to concentrate all his attention on the observation, and the difference between the rate of the auxiliary clock and the rate of the transit clock (which it is in the power of the superintendent to adjust) causes the signals to occur at different portions of the seconds shown by the transit-clock.

"In circumstances which permit the record of clock-seconds and observations, or signals by punctures produced by a galvanic magnet upon a revolving disk or barrel, or the transmission of transits by a galvanic wire, a simpler method may be employed."

The section closes with a brief journal of the operations.

1854MNRAS..14..246J

Section II. *Comparison of Observations of the Galvanic Signals by different Observers.*

"As it was intended by the interchange of observers to eliminate the errors arising from personal equation, as well of signal observation as of transit observation, no particular effort was made to obtain a comparison of the various modes of observation. No efficient comparison of the two real observers (Messrs. Bouvy and Dunkin) could have been made without a most inconvenient extension of the operation."

A few comparisons were, however, incidentally made between the observations of Mr. Bouvy on the one hand, and those of two of the Greenwich Assistants on the other. The author concludes this section with a synopsis of the mean results, and the extreme ranges on each side.

Section III. *Comparisons of the Recorded Clock-Times of Galvanic Signals observed at Brussels and Greenwich, which are not accompanied with Astronomical Observations.*

"The signals which were not accompanied by observations of transits are useless for the determination of difference of longitude. They are, however, perfectly available for determining the time occupied by the passage of the galvanic current from Greenwich to Brussels, or *vice versâ,* as will appear from the following explanation of the method of treating them.

"In the first series, the signals of the first quarter of each hour were given exclusively by Greenwich contact near the Greenwich battery. Let t be the time occupied by the passage of the current: then, during the first quarter, the times read from the Greenwich clock will not be increased from this cause, but the times read from the Brussels clock will be increased by t. The reading of the Brussels clock is (in consequence of its eastern longitude) usually greater than that of the Greenwich clock. Suppose that the excess as unaffected by time of transmission ought to be found $= E_1$, but that as affected by time of transmission it appears (on comparison of observations) to be e_1, then the comparisons of the observations of the first quarter of an hour give this equation,

$$E_1 + t = e_1$$

where E_1 and t are yet unknown.

"In the second quarter of each hour the signals were given exclusively by Brussels contact near the Brussels battery. In this case the Brussels clock readings were not increased, but the Greenwich clock readings were increased by t, and, therefore, the excess of the Brussels clock readings was diminished by t. Hence these comparisons give

$$E_2 - t = e_2$$

"Similarly the third quarter gives

$$E_3 + t = e_3$$

1854MNRAS..14..246J

and the fourth quarter,

$$E_4 - t = e_4$$

" If the two clocks had no relative rate, the four numbers, E_1, E_2, E_3, E_4, would be equal. As it is certain, however, that there was a relative rate, the only supposition that we can venture to make is that it was uniform during the hour. We may also in most cases assume that the mean of the signals in each quarter of hour corresponded (with sufficient approximation) to the middle of the quarter of hour. Let the relative rate for one quarter of hour be r, then the equations become,

$$\begin{aligned} E_1 \qquad\quad + t &= e_1 \\ E_1 + \quad r - t &= e_2 \\ E_1 + 2r + t &= e_3 \\ E_1 + 3r - t &= e_4 \end{aligned}$$

" The easiest way of treating these equations is the following:

" Subtract the double of the second equation from the sum of the first and third:

$$4t = e_1 + e_3 - 2e_2, \text{ or } t = \frac{e_1 + e_3 - 2e_2}{4}.$$

" Subtract the sum of the second and fourth equations from the double of the third:

$$4t = 2e_3 - e_2 - e_4, \text{ or } t = \frac{2e_3 - e_2 - e_4}{4}.$$

" The mean of these two determinations may probably be adopted as preferable to either of them alone.

" In the second series, when the signals in the first and third quarters were given from Brussels, the process is the same, but the sign of t must be changed."

The section concludes with the results of the individual day's observations.

Section IV. *Authorities for the Instrumental Errors and Clock Errors on the Days which are available for Determination of Difference of Longitude.*

This section contains an account of the methods used for the determining the coefficients of instrumental and clock correction; and also so much of the numerical values of those coefficients as will enable the reader to judge of the stability of the instruments and the general trustworthiness of the results.

Section V. *Comparison of the Sidereal Times of Galvanic Signals observed at Brussels and Greenwich, on the Days which are available for the Determination of Difference of Longitude.*

" The observed transits were corrected numerically for the effects of the instrumental errors of which the elements are given

1854MNRAS..14..246J

in the last section. The clock-errors were obtained by comparing the corrected transits with the tabular places of the stars specified in the last section; and these clock-errors, duly reduced to the time of signal observation, were applied to the recorded clock-times of signals. The difference of these at the two observatories gives the apparent Difference of Longitude. The results for Difference of Longitude in the separate quarters of hour are treated in the same manner as the simple clock comparisons in Section III., in order to obtain from them the time occupied by the galvanic current in passing between Brussels and Greenwich. For the Difference of Longitude (still subject to the effects of personal equation) it is necessary to take the mean of the four separate results."

The author then gives the numbers obtained on each day by the two independent methods, A and B.

Section VI. *Abstract of the Results for the Time occupied by the Passage of the Galvanic Current and for the Difference of Longitude.*

The definitive result for the time occupied by the Galvanic Current in passing between the two observatories is found to be $0^s{\cdot}109$. This evaluation is based upon 2616 observations.

"The result, $0^s{\cdot}109$, may arise from the sum of two quantities: first, the time really occupied by the passage of the galvanic pulse; secondly, the comparative slowness of perception of the observer at the distant station, arising from the more languid motion of the needle. But having been fully able to appreciate the attention with which the observers watched the needle, and the intentness with which their energies were directed to seize its first movements, —remarking also that, in consequence of the admirable insulation of the telegraph wires, the difference in the intensities of the current at the near and distant stations is very small,—I am disposed to think that the number $0^s{\cdot}109$ represents almost purely the time employed by the transit of the galvanic current.

"The telegraphic distance between Brussels and Greenwich is about 270 miles. If the velocity of the current were uniform, the value obtained from these experiments would be little more than 2500 miles per second, or not more than $\frac{1}{7}$ of that obtained by several American investigators. It is, however, to be remarked, that from Greenwich to London and thence to Ostend, the whole of the line is subterraneous or subaqueous; and that unpublished experiments by Mr. Latimer Clark (which, by the kind permission of that gentleman, I have myself witnessed) show that the velocity in such wires is not greater than 800 or 1000 miles per second. It appears, therefore, extremely probable that the retardation which we have discovered belongs almost entirely to the subterranean and submarine portion of the line; and that the retardation between Ostend and Brussels is practically insensible."

The final results for the Difference of Longitude, as determined by the two methods A and B, are the following:

1854MNRAS..14..246J

	Method A.		Method B.	
	m	s	m	s
Mean of first series......	17	29·256	17	29·340
second series....	17	28·538	17	28·476
Mean of the two series ..	17	28·897	17	28·908

These determinations rest upon 1104 signals. The author concludes with the following remarks:—

"The last result, or $17^m\ 28^s{\cdot}9$, is indisputably the best that can be given at the present time for the Difference of Longitude of the two Observatories. The difference, however, of the results given by the two series, $0^s{\cdot}791$, is somewhat striking. If it be attributed entirely to personal equation, it implies, either that Mr. Bouvy registers signals earlier by $0^s{\cdot}4$ than Mr. Dunkin would do in the same circumstances, their registers of transits of stars being identical; or that Mr. Bouvy registers transits later by $0^s{\cdot}4$ than Mr. Dunkin would do, their registers of signals being identical; or that Mr. Bouvy registers signals somewhat earlier and transits somewhat later, the sum of the two discordances amounting to $0^s{\cdot}4$. Neither of these explanations is impossible, and the second is supported by the remark which M. Quetelet has communicated to me, that Mr. Bouvy registers transits later by $0^s{\cdot}3$ or $0^s{\cdot}4$ than M. Quetelet does, while there is no sensible difference between their modes of observing signals. The first suggested explanation is not supported by the observations recorded in Section II.

"The only remaining explanation of the difference of results of the two series, which appears possible, is, an error in the evaluation in one or both series of the instrumental errors. I have endeavoured to give all the means for decision on this point, and must leave it to the judgment of the reader.

"When the American* method of recording transits by galvanic punctures upon a revolving plate or barrel shall be more extensively introduced into Europe, I think it probable that operations equal in value to that described in this paper may be effected in a shorter time. But, even with the signals of the telegraph needle, repeated for a few days, the accidental errors of signal observation may be considered as practically eliminated, and those of transit observation will also sensibly vanish. The only remaining sources of error are, the personal equations and the inaccuracy of instrumental adjustments: the former may be eliminated by interchange of observers, but for the latter only the most careful attention to the principle and the practice of the methods employed will suffice."

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* The author here appends the following foot-note:—"I use this term as implying that the method in question was practically introduced and extensively applied in America before it was employed in any other country. I know not by whom or where the method was first proposed."